inches face, of 60°, and the spectrum will be brought to a focus on a sensitive plate by a lens of a mean focus of 9 inches. An attempt will be made to secure the whole of the spectrum, and for this purpose the plate requires to be inclined at an angle of 40° to the axis of the lens. The spectrum which it is hoped to obtain by this arrangement would have required an hour's exposure some years ago.

With this instrument attempts will be made to secure a photograph of the flash of bright lines at the beginning of totality, and the spectrum of the corona during totality, an arrangement being made for a comparison solar spectrum after totality by shutting off half of the slit.

So much, then, for the work suggested by applying views and methods which have been broached since the last eclipse.

The remarkable form of the corona, and its still more remarkable extension in 1878, and its great variation from that seen in prior eclipses with a single exception—that of 1867—will render observations of the form and extent of the corona in the present year of the highest importance, even if we had not Dr. Siemens' suggestive hypothesis to lend a more than usual interest to it.

I give an illustration (Fig. 1) copied from the American volume, which I owe to the courtesy of the Superintendent of the Naval Observatory at Washington, to show how my own observations of that eclipse, an account of which was sent to NATURE from America at the time, have been borne out by a discussion of the photographs. Side by side with it, in order that the equatorial extension and the almost identical tracery at the poles can be seen, I give a copy of a drawing made in 1867, both sets of observations having been made four years before the sun-pot maximum (Fig. 2).

Here, indeed, we have food for thought; for the currents in the solar atmosphere, revealed by these drawings, seem to be exactly those demanded by Dr. Siemens; and indeed, his hypothetical diagram which appeared in NATURE a few weeks ago, should be compared with them, in order that the points of resemblance may be grasped.

With reference to the other drawing (Fig. 3), which shows the remarkable observation made by Prof. Newcomb, I cannot do better than make the following quotation from the volume in question:—

"It had always seemed to me that the visual study of the faint outlying portions of the corona would necessarily be interfered with by the brilliant interior portions unless the view of the latter were intercepted. I therefore made preparations to repeat the experiment unsuccessfully attempted at Desmoines in 1869, of hiding the central corona by a screen about 1° in diameter, and examining such portions as might be visible outside of it. The screen now used was made of wood, about 12 inches in diameter, and was mounted on top of a telegraph pole which was set on the elevated ground to the west. The altitude and azimuth of the sun at the moment of central eclipse were carefully calculated, and the screen fixed in such a position that when viewed from the top of a stake driven in the ground alongside of my telescope it would cover the eclipsed sun. The angular diameter of the screen as measured with a sextant from the stake was 57′, its distance was about 60 feet. As this would cut off about 12′ of the corona all round the moon I considered it ample for the purpose, but the results showed that it might well have been somewhat larger.

"I remained in the dark room until about three minutes before the commencement of totality with the view of having my eyes as sensitive as possible. I then walked to the telescope, keeping my eyes partially protected from the light. The lurid colour of the landscape was very striking. The light seemed no longer to be that of the sun but rather to partake of the character of an artificial illumination. This appearance is very readily explicable by the fact that the light coming only from the limb of the sun belongs principally to the red end of the spectrum. As the last ray of sunlight was disappearing I stepped to a stake driven into the ground, the top of which marked the point from which the sun would be entirely hidden by the screen. A bright corona was plainly visible all round the screen, although a portion 12′ from the limb of the sun was entirely cut off. My attention was immediately attracted by a faint blush of light, extending out on each side at an angle of about 45° with the horizon, each end terminating in a long narrow ray. I made a very careful estimate of the length of these rays as 6° from the disc. They shaded off by insensible gradations, and struck me as having a great resemblance to a representation of the zodiacal light on a reduced scale. They were to all appearances continuous with the corona. With a view of judging whether their direction coincided with that of the ecliptic, I tried to judge whether the western one pointed towards the planet Venus, then plainly visible near the horizon. Its direction was apparently very slightly below that of the planet.

"The outlying portions of the corona other than those rays were extremely irregular; that is, there were several rays and other irregularities extending out in different directions. As these were common phenomena, I took no note of their details." J. NORMAN LOCKYER

April 18

(*To be continued.*)

PROF. WIESNER ON "THE POWER OF MOVEMENT IN PLANTS"

Das Bewegungsvermögen der Pflanzen: eine kritische Studie über das gleichnamige Werk, von Charles Darwin, nebst neuen Untersuchungen. Von Julius Wiesner. 8vo, pp. 212. Three Woodcuts. (Wien: Hölder, 1881.)

BEFORE attempting to reply to some of Prof. Wiesner's criticism, it is a pleasure to record my appreciation of the courteous spirit in which his book is written, and the uniformly respectful tone which he employs towards my father. His criticism is so extensive that there is hardly a single point of any importance in "The Power of Movement in Plants" with which Prof. Wiesner agrees. Yet in spite of this far-reaching difference of opinion, he is good enough to express himself warmly as to the value which the book possesses.

Wiesner devotes a good many pages to Circumnutation, and as this phenomenon and the theories connected with it form an important part of "The Power of Movement in Plants," I shall begin with this question. In the first place Wiesner finds fault with one of the methods employed by us in our observations on circumnutation, and gives a diagram (Fig. 3, p. 161) which shows that the method may lead to false conclusions. In the method of observation criticised by Wiesner, the position of the plant at any moment was determined by making a dot on a glass plate in such position that it was in a line with a mark on the organ whose movements were to be observed and with a stationary mark behind or below it. This method is obviously open to objections, and we never imagined it to be strictly accurate ; but Wiesner shows that it is possible, by taking some pains, to make it very inaccurate.[1] But the arrangement given in Wiesner's diagram is one which no one would think of employing, if he wished to make the best of the method ; in all our experiments we tried, as far as possible, to avoid the extremely oblique arrangement chosen by Wiesner. This may to a large extent be ensured by placing the fixed mark as near the base of the plant as possible, *i.e.* when tracing on a horizontal plane the movements of a vertical organ ; if when the stem of the plant is vertical, the index attached to the plant is vertically above the fixed mark, then growth vertically upward will be represented by a single dot. By taking reasonable pains in making some such arrangement, I still believe that no very serious error will be introduced.

In the second method of observing circumnutation, a glass filament bearing two small sights was fixed to the plant, and its position was recorded by making a dot on a glass plate in line with the two sights. Against this method no such serious charges can be brought, and it was largely used, and was, as matter of fact, preferred by us. The methods given by Wiesner are in many ways, no doubt, preferable to those employed for "The Power of Movement," and are in principle the same as that described by me in the *Bot. Zeitung*, 1881, p. 473, which consisted in estimating the actual position of the moving point by means of a vertical microscope; Wiesner has also employed a vertical tube without lenses. In the latter case, the position of the tube is varied until the cross wires are vertically over the observed point, and the various positions of the cross wires at successive intervals of time can be recorded in a way we need not stop to describe. In the case of the microscope the movements are recorded by means of the eye-piece micrometer. This method requires to be treated fairly and not to be burlesqued ; it presupposes a knowledge of the general direction in which the growth of the organ under observation is proceeding, and the microscope should be parallel to this direction. If as in Fig. 1 a plant grows straight from *a* to *b*, it will be seen by the microscope M, moving in the direction of the arrow ;[2] growth from *b* to *c* will not be perceived as lateral movement, but as growth towards the observer, and the same is true *mutatis mutandis* for the microscope N. Thus if we estimate the whole lateral movement which has taken place during the growth from *a* to *c* by the two microscopes M and N, we shall see that they give reverse results. It will therefore be seen that the same general knowledge of the direction of growth is required for Wiesner's and for our method, and that unless this knowledge is properly utilised, either method can be made to give wrong results.

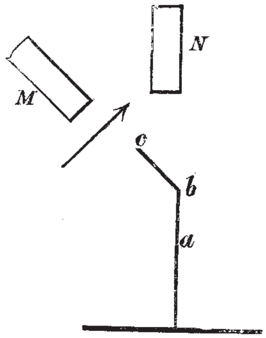

FIG. I.

FIG. 1.—Diagram representing a plant which is supposed to increase in length by the portions *a b*, *b c*. M and N represent microscopes for observing the direction of growth.

In a notice like the present, it is impossible either to give or to attempt to answer all Wiesner's criticisms, and in what follows I cannot do more than notice what seems to me the more important points. Wiesner states that circumnutation is not nearly so general a phenomenon as we believe it to be. That growth in a perfectly straight line (with a qualification to be mentioned hereafter) is found to occur, and therefore that circumnutation is not an essential quality of growth, is not, in fact, an "Urbewegung" (Primordial-movement).

Let us first consider the circumnutation of roots. The observations given in "The Power of Movements" on this head were made by two methods. In some cases a glass fibre (P. of M., p. 10) was fastened by shellac-varnish to the tip of the root, and the movements of the end of the glass fibre were then recorded by making dots on a glass plate. In other cases the tip of the root was made to inscribe its course on the smoky surface of an inclined glass plane. By this means curious wavy and broken lines were drawn on the glass plates, which we believe to afford evidence of circumnutation. Wiesner confirms the results, but differs entirely in the conclusions which he draws. He believes that the coating of soot is the cause of the apparent circumnutation. He believes that the soot acts injuriously on the root, and causes it to curve away from the injured side, by means of the specialised sensitiveness which, as we have shown, enables a root to curve away when the tip is injured by caustic, &c. He supports this view by an experiment in which (Wiesner, p. 166) the inclined glass plate was coated with *semen lycopodii* instead of soot, and he always found that the course described was a straight unbroken line. This experiment is strongly in favour of Wiesner's view ; but, on the other hand, I fail to see how Wiesner's explanation applies to the lateral movements of the root[1] which gives the waviness to the course traced, although it may legitimately be used to explain the movements away from the smoky surface which cause the line to be often a broken one.

The other observations on the circumnutation of roots recorded in "The Power of Movement" are set aside by Wiesner among other reasons, because they were made by fastening a glass filament to the tip of the root, a method which, as he states, disturbs the growth of the root and causes an apparent circumnutation. Moreover, Wiesner's own observations, made with a microscope, lead him to disbelieve in the existence of circumnutation in roots. Wiesner says (p. 174) that the circumnutation of roots is due to the antagonism between geotropism and the natural tendency to curvature existing in the root (Sachs' curvature). He believes that first one and then another of these forces gets the upper hand, so that the tip of the root moves backwards and forwards.

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[1] It may, however, be observed that if the plant in Wiesner's diagram had grown straight on in the original direction, the tracing given would have been a straight line, and we should have drawn the correct conclusion that the plant was not circumnutating.

[2] Or rather in the reverse direction, owing to the reversal of the image by the microscope.

[1] The lateral movements are probably explained by Wiesner as the result of the antagonism of geotropism and that tendency to nutation which we call Sachs's curvature.

To this explanation there are several objections. (1) If the root is pointing vertically upwards, as in some of Wiesner's experiments (p. 174), any tendency to spontaneous curvature in the root will assist geotropism, so that any circumnutation that may occur is inexplicable as the result of antagonism of the above-named forces. (2) Wiesner states (pp. 169, 174) that the movements did not take place in one plane: his explanation does not account for this fact. (3) He states (p. 172) that the "so-called" circumnutation takes place in the part of the root which grows most quick. But "Sachs' curvature," which he assumes to be one element in the "so-called" circumnutation, takes place at the base of the root. It is true that it may be said in favour of Wiesner's view that the root may be carried out of the vertical by Sachs' curvature; and if this were the case geotropism would bring it back to the vertical, and thus the direction of the circumnutation would correspond more or less with the plane of Sachs' curvature. But this would not account for movement in any other plane.

Circumnutation of Stems.—Wiesner concludes that there exist stems of plants which certainly do not circumnutate at all. This statement he founds (pp. 176, 177), however, on observations on plants whose line of growth is not a straight line, but is broken by lateral oscillations in various directions. The lateral movements being small and irregular, however, are held not to constitute circumnutation. In one case the growth of the grass seedlings under observation was at first accompanied by the above-mentioned very minute and irregular curvatures, but afterwards seems to have circumnutated in our sense of the term. I shall return again to these cases.

In the case of stems which show the S-shaped curve of "undulating nutation," Wiesner observed the tip (Faba, p. 178) move backwards and forwards in the plane of curvature; this he explains as the summation of the apogeotropic curvature of the lower part of the stem with the nutation (*i.e.* curvature) of the upper part.

Unless I misunderstand Wiesner in this point, it seems to me his explanation does not meet the facts; for I fail to see how *summation* of two curvatures can produce anything, except a variation in the rapidity of the curvature. I cannot see how it accounts for any movement in the opposite direction.

Nor again does Wiesner give any explanation of the movements which he observed in the plane at right angles to the nutation-plane. It should, however, be mentioned that in the epicotyl of the bean, Wiesner observed perfect straight growth after the undulatory nutation had ceased (p. 178).

Wiesner's observations on heliotropism (p. 182) in connection with circumnutation do not call for any special remark. He seems not to have taken the precaution to expose the plants experimented on to a *dull* light, and the plants consequently curved in nearly or quite straight lines towards the light, as occurred in our experiments.

The movements of the flower-head of the daisy (p. 183) Wiesner puts down to the effects of the weight of the flower-head. He also assumes that the circumnutation which he observed in a flowering spike of a Plantago is due to the irregular disposition of the florets on the inflorescence.

Circumnutation of Leaves.[1]—On this point Wiesner's views are briefly:—(1) Some leaves grow in absolutely straight lines without circumnutating. (2) He confirms the *facts* observed by us, namely, that the tip of the leaf does describe the complicated figures described by us as circumnutation, but he interprets the facts differently. He believes that the complicated forces acting on the leaves, viz. epinasty, apogeotropism, apheliotropism, influence of weight, &c., working in antagonism to one another, and alternately getting the upper hand, produce the movements in question.

This argument is one of the most important which Wiesner makes use of, and a careful consideration such as it deserves would require further experiment and observation. It is obviously difficult to distinguish between circumnutation modified by the contending forces, and the same contending forces acting on an organ without circumnutation. One feature in our observations is the almost constant presence of movements in a horizontal plane, movements therefore which cannot be produced by any of the contending forces above described. Against the numerous cases in which sideway movements occur, it may be mentioned that, according to Wiesner, cases occur in which no lateral movements can be observed (p. 192).

This will perhaps be a convenient place to discuss the minute irregular disturbances which Wiesner usually found to exist even where the organ did not properly circumnutate. Let us for a moment compare circumnutation with variability. Modifications of organs are brought about by the summation of small variations in certain directions, and thus we rightly consider variability as the necessary groundwork for modification. But we find some animals, *e.g.* the common goose, which may be almost said not to vary, yet we do not, on the strength of this fact, assert variability is not necessary for modification. No two organs are mathematically similar, yet we cannot draw a distinction between minute irregular deviations from the normal, and such plain deviations as are called variations. In the same way it may be said that no fast and firm line can be drawn between circumnutation and the minute irregular disturbances of Wiesner. We have shown that a true circumnutating movement (as in Brassica) is made up of very small irregular jerks, and this may be given as another reason for believing that the two kinds of movement are only extreme forms of the same phenomenon.

In summing up what he has to say on the subject of circumnutation, Wiesner says (p. 202) that the movements described by us as circumnutation are either disturbances of growth or they are produced by combinations of antagonising forces, *or they are identical with the revolving nutation of climbing plants.* From Wiesner's brief manner of dismissing the last mentioned class, it might be supposed that it has little or no bearing on the question. But this is far from being the case; it is precisely this class to which we attach importance. There can be no doubt that revolving nutation of climbing plants is a development or exaggeration of circumnutation. In the stolons of the strawberry we have a mode of growth which may almost be said to be halfway between circumnutation and the revolving nutation of climbing plants. The scattered distribution of climbing plants throughout the vegetable kingdom proves the wide distribution of a form of growth from which revolving nutation is developed—and this form of growth is circumnutation. This widespread form of circumnutation cannot therefore be dismissed as Wiesner has done, since such a treatment of it is quite beside the question.

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[1] It is curious that Wiesner (p. 186) recommends the use of glass fibres affixed to a stem for observing circumnutation, while on p. 187 he suspects that circumnutation of leaves is disturbed by attached glass fibre.

General Mechanism of Movement.—In "The Power of Movement" we have spoken of the movements of plants as being due to difference in turgescence and in ductility on opposite sides of the moving organ. In it we have pointed out that it is more correct to look at the difference in turgescence than to difference of growth as the primary cause. In this statement "growth" was meant to mean alteration in size due to intercalation of solid particles. If a turgescent stem is allowed to bend heliotropically and is then placed in a salt solution strong enough to destroy its turgescence the heliotropic curvature is in large part destroyed; showing that the curvature at any given moment is largely due to differences in turgescence. Wiesner (p. 33) made the following experiment: he took five seedlings and found that when placed in salt solution (10 per cent.) they became shorter on the average by 1·9 mm.; five other similar seedlings he allowed to grow for 4½ hours, during which time they increased in length by 6·2 mm. (average), Wiesner argues that if the whole increase in length during 4½ hours were due to turgescence, then the shortening caused by salt solution ought to = 6·2 mm. + the original 1·9 mm. which was shown to be in a turgescent state by the former experiment; this, however, was not the case. It would be almost as reasonable to measure the length of an epicotyl in a dormant seed, and to expect that after germinating and growing for a day or two the young plant should collapse in salt solution to the size of the rudiment which existed in the seed. I imagine that it is a generally received opinion, and one which does not require Wiesner's experimental demonstration, that increase of length by turgescence and the intercalation of solid matter proceed simultaneously.

The question, however, need not be considered in further detail, for on this point there is practically no difference between Wiesner's and our own view; he says (p. 35) that "growth is from the first a combination of several processes occurring simultaneously, of which however Turgor is at first the governing one (*vorherrscht*)." This view is the same as that of Sachs ("Lehrbuch," Eng. Tr., p. 712), who describes the interaction of turgescence and intercalation.

Heliotropism.—Wiesner's criticism on the new matter contained in our book with regard to Heliotropism is prefaced by a discussion of some length on the nature, &c., of the phenomena of Heliotropism. There is only one point in this discussion which I wish at present to call attention to. Wiesner holds to De Candolle's explanation of heliotropism, namely, the purely mechanical view that the convex side grows more quickly, simply because it is in shade. As this view does not account for apheliotropism since at least some apheliotropic organs also grow more quickly in darkness than in light, Wiesner assumes (p. 55) the existence, in the fibro-vascular bundle, of negatively heliotropic elements, whose growth is assumed to be favoured by light. It is a pity that this theory has not been at least partly tested by comparing the rate of growth of unicellular apheliotropic organs in light and darkness.

Again the theory seems to require that all apheliotropic organs should be positively heliotropic in light o low intensity; and this, as far as I know, is not the case with the roots of *Sinapis alba*, which I have shown ("Arbeiten des Bot. Inst. Würzburg," Bd. ii., Heft. 3) to be apheliotropic even with very dull light. But I do not lay great stress on this argument, as Wiesner's theory seems to me to rest too much on assumptions to be at present capable of being discussed.

Transmission of Heliotropic Stimulus.—In "The Power of Movement" (Chap. ix.) facts were given that seemed to us to show that one part of an organ may bend heliotropically owing to the illumination of another part, and that, therefore, a heliotropic stimulus is transmittable from one part of an organ to another. The experiment which led to this conclusion is as follows:—A number of small glass caps were prepared, some of which were left transparent, the others were painted black. These caps were then slipped over the tips of seedlings of Phalaris (canary grass), which were thus prevented from bending towards the light, although they did so when the unpainted glass caps were used. The same experiment was made with other seedlings—cabbage among the number. From these results it was inferred that the illumination of the upper part of the plant was necessary to make the lower part bend towards the light; or, in other words, that the heliotropic curvature of the lower part depends on some influence transmitted from the upper and illuminated part. In the case of cabbage seedlings we found that if the lower part is darkened, while the upper is illuminated, no heliotropic curvature of the lower part takes place. Thus we believed that the lower part is in some degree heliotropic, independently of the illumination of the upper part, although the latter is the most important factor in the illumination of the plant. This seems to have been misunderstood by Wiesner, whose criticism is largely directed against what he believes to be our view, namely, that a heliotropic stimulus can be transmitted to a part of an organ which is not itself heliotropic. This misunderstanding on the part of Wiesner is no doubt due to a want of clearness in what we have said on the subject.

Wiesner speaks with great positiveness on this subject, and asserts that he has shown to demonstration that our experiments do not justify our conclusions. He believes that the bending of the lower part of the plants observed in our experiments is due to what he calls "Zugwachsthum" (p. 72), that is to say, the effect of the weight of the upper part of the plant acting on the growth of the lower part. We have discussed the question whether the curvature of the lower part of the cotyledons of Phalaris can be due to weight, and have shown conclusively that it cannot be so ("Power of Movement," p. 469), but Wiesner makes no mention of this experiment. Pots of Phalaris seedlings were placed on their sides so that the cotyledons were horizontal and were at right angles to the direction of the incident light from a bright lamp. Under these circumstances they became bent close to their bases, nearly the whole cotyledon being thus directed towards the light.

This experiment I have repeated since reading Wiesner's book, and have found the results to be the same. The conclusion is inevitable and is in this case absolutely destructive of Wiesner's theory of "Zugwachsthum."

This theory he grounds on the following experiment (p. 69), in which he makes use of Sachs's method of observing heliotropism:—Seedlings growing in small vessels are fixed in the place of the minute-hand of a large clock, so that each seedling is at right angles to the axis of rotation, and rotates like the hand of the clock; they are then illuminated by light which is parallel to the axis of rotation, and therefore each seedling has one side constantly illuminated by light striking it at right angles. Owing to the constant rotation the effect of weight is eliminated, and thus any curvature which occurs cannot be due to "Zugwachsthum." Wiesner states that whereas the seedlings on the klinostat (Sachs' name for this instrument), were only curved in their upper parts; plants growing normally without being subjected to rotation were curved down to the ground. This seems at first a conclusive argument against our view, but I shall show that in the case of two plants, cabbage and Phalaris, it is not so.

We expressly stated (p. 479) that our experiments on cabbages were made on young seedlings "about half an inch or rather less in height," because when the plants have grown to an inch and upwards in height the lower part ceases to bend heliotropically. Now Wiesner's experiments were made confessedly on seedlings whose lower part was growing slowly, and which were therefore probably older than those which we employed for our experiments. When Wiesner made his rotating experiment with *young* cabbage seedlings they became curved down to the ground. This proves that the curvature which occurs near the ground in young cabbage seedlings is not due to weight; and this is the very curvature which we have shown not to occur unless the upper part is illuminated. I do not attempt to explain Wiesner's experiments on old cabbage seedlings, but those made with young ones are alone of importance for us, and they are conclusively on our side.

With regard to Phalaris I regret that I cannot confirm Wiesner's results, who states that these seedlings behaved like the dicotyledons experimented on; *i.e.* that when grown on the rotating apparatus they do not become bent down to the ground. I have experimented with young seedlings such as we should have used for the experiments on transmission of the light-stimulus, and found that many of them became well bent down to the ground. But it should be remarked that in some cases a certain amount of difference in this respect was observable between the plants on the klinostat and normal ones.

FRANCIS DARWIN

(*To be continued.*)

OUR BOOK SHELF

Through Siberia. By Henry Lansdell. Two volumes, with illustrations and maps. (London: Sampson Low and Co., 1882.)

IT is obvious that much scientific information cannot be expected from a traveller who was, to use his own expression, "flying across Europe and Asia," and who crossed Siberia from Ekaterinburg, in the Ural Mountains, to Tobolsk in the North, Barnaoul in the Altai, and Nikolaevsk on the Pacific, a distance of 6600 miles, in seventy-eight days, and whose aim was, during this very short time, to investigate the situation of Russian prisons. The author has, however, supplemented his own somewhat superficial observations by information obtained from good sources. The book is provided with many illustrations, partly taken from other works (without quoting the source from which they are taken), and partly from new photographs. These are sometimes very good, but sometimes they convey quite false ideas, as, for instance, the photograph of a "Buriat girl," who obviously is a metis, having very little in common with true Buriats.

P. K.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Limulus

IN a criticism published in the *American Naturalist* for April, 1882, on Prof. Ray Lankester's recent most able memoir, entitled "Limulus an Arachnid," Mr. A. S. Packard, whose most important researches on Limulus are familiar to all zoologists, and to whose courtesy I am indebted for a copy of his criticism, after stating other grounds which lead him to differ in opinion from Prof. Lankester as to the close relationship of the King Crab and the Scorpion, quotes in his final paragraphs extracts from published letters written by my late lamented friend and shipmate, R. von Willemoes-Suhm, from on board H.M.S. *Challenger*, at the Phillipine Islands and Japan in February and May, 1875, concerning certain Arthropod embryos which he had had under observation at Zamboangan, and which he then supposed to be the larvæ of *Limulus rotundicauda.* As Von Suhm and I worked together for more than two years daily with our microscopes within two feet of one another, we naturally discussed all that we did and observed in common, and we frequently talked about these supposed Limulus embryos, and looked at them together. It is as well, therefore, since the statements concerning them are being made use of to assist in disproving the position assumed by Prof. E. van Beneden, Prof. Lankester, and others as to the Arachnid nature of Limulus, a position of the strength of which I am myself persuaded, that I should state in print, that long before his death Von Willemoes-Suhm was completely convinced that he had been misled as to the larvæ, and told me that he felt sure they were not those of Limulus at all, but belonged to a Cirrhiped of some sort. I some time ago told my friend, Prof. E. van Beneden, who inquired on the matter, that such was Von Suhm's final conclusion. And I also long ago told Prof. Lankester, and this is no doubt the reason why no reference to Von Suhm's letters was made by the latter in his memoir.

It must be remembered that the only evidence in favour of Von Suhm's Nauplius larvæ being those of Limulus, lay in their general appearance, which simulated to some extent that of an adult Limulus, and in the fact that they were caught with the tow-net in Zamboangan harbour, a locality at which *Limulus rotundicauda* occurs.

H. N. MOSELEY

Oxford, April 15

Silurian Fossils in the North-West Highlands

THE publication of Dr. Heddle's geological and mineralogical map of Sutherland, which was noticed in NATURE, vol. xxv. p. 526, calls to mind some curious points with reference to that region—points on which we should like to have some further and more definite information.

Dr. Heddle quite acquiesces in the general accuracy of the stratigraphical conclusions arrived at by Murchison and his colleagues, and, as may be gathered both from his map and writings, has seen no cause whatever to induce him to believe either in the great fault of Prof. Nicol, or in the unconformity alleged by Dr. Hicks to exist in the adjacent county.